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THE ALLEGED PROOF OF PARALLELISM FROM THE CONSERVATION OF ENERGY.

THE earlier protagonists of the doctrine of psycho-physical parallelism were content to rest their case upon a rather complicated argument from general analogy. The theory presented itself as an hypothesis which seemed to be demanded by the continuity between reflex action and conscious voluntary action, and by the known facts of mental pathology. Its general accord with some of the most vital tendencies of scientific thought, as well as its simplicity and elegance from a philosophic standpoint, were additional reasons for its acceptance. But since its practical adoption as the working hypothesis of modern psychology, a more summary method of proof has become fashionable. Like many scientific points of view originally established by careful induction, it has become an *a priori* principle. We are now told that it follows necessarily from the principle of the conservation of energy, and to doubt it is to doubt the "grandest generalization of modern science." Into this argument it is my purpose to inquire. It will be my endeavor not merely to expose the fallacy of the bare argument as given, but, if possible, to make the discussion more psychological—to get at the real origin of this curious belief. I shall make my best effort to get at, and answer, the real thought that underlies the argument, and not merely the argument itself as it stands. If I do not succeed in doing full justice to the supporters of the view, it will not be from failure to make every effort to see the problem as they see it. I trust then, whatever else I may be accused of, I shall not be accused of wilfully misrepresenting the argument I am attacking.

We may best approach the subject by taking a classical example of what may be termed an over-interpretation of the principle of the conservation of energy. It is frequently said that the laws of electro-magnetic induction can be deduced from the conservation of energy; and this is sometimes mentioned

as an instance of the wide application and extreme usefulness of the law. It has been shown, however, that this statement is not entirely true, and that as a matter of fact other principles are assumed in the alleged deduction which are by no means certain *a priori*.¹

Let us take a simple case of the problem. We have two electric conductors carrying currents which attract each other. If now they are free to move, they will approach each other. That is, a mass of matter will acquire a velocity—kinetic energy has been produced. Now the principle of conservation tells us that this energy must have come from somewhere; there must be a corresponding diminution somewhere else. We are, therefore, forced to conclude that the approach of the conductors has led to a diminution of the current strength of each. That is, a reverse electro-motive force has been produced in each by the approach of the two circuits.

No, says the critic, not so fast. The first part of your statement is all right. Energy comes from somewhere. But how can you be sure that it comes from just where you suppose? Is it not possible that the current strength remains the same, but that under these circumstances the same current produces less heat in the conductors? How do you rule out the possibility that the battery, under these circumstances, acts more energetically? In fact are there not, *a priori*, an almost infinite number of possible sources of the kinetic energy that is produced, which only experience will enable you to rule out?

It is not necessary to follow this discussion further. Some of the alternatives proposed have been shown to be rather improbable, others have seemed very probable. For our purpose it is sufficient to point out that the principle of the objection is valid. The law of the conservation of energy never enables us to tell what becomes of the energy, but only that it still exists SOMEWHERE. If a moving body strikes another, we know that its energy will not be destroyed; but to know what does become of it we must appeal to the *experimentally established laws* governing the collision of bodies. How much of it will remain as

¹ Cf. Poincaré, *Electricité et optique*, II, p. 25.

kinetic energy in the body, how much be passed on to the body struck, how much be transformed into heat and other forms of energy—these are questions quite distinct from the mere question of conservation, and can be settled only by appeals to other principles. The conservation of energy is a single equation between several unknown quantities, and to try to find the value of any one of these from the single equation is absurd. Yet it is just this that one pretends to do, when he asserts that the conservation of energy demands that at a certain time, a certain quantity of energy must appear in a certain form, at a certain place. The equation $x + y = 5$ cannot be contradicted by any statement as to the value of x . Nothing can contradict it but an express statement that the sum of x and y is not equal to 5, or is equal to some other value. Similarly, the principle of the conservation of energy cannot be contradicted by any single statement as to the appearance or disappearance at any place of a quantity of energy, but only by an express statement that the *total quantity* of energy in the *universe* has been increased, or that energy has appeared at a certain point which has *not* come from elsewhere. The principle of the conservation of energy is only one of the laws governing the transformation of energy. To ascertain what transformations will take place in a particular instance, we have to know other laws applying to the particular case.

We may now gauge at its proper value the assertion that the freedom of the will—that is, a freedom manifested in the phenomenal world—is inconsistent with the conservation of energy. Such a proposition confuses two totally different questions. The question of freedom has to do with the laws governing the time and place of the transformation of energy, not with the quantity of the same. The conservation of energy would not in any way be interfered with, if there were an absolutely free alternative in any case as to whether a given quantity of energy should retain its form or be transformed, provided that the quantities were the same in each case. It might, for example, be an absolutely unpredictable affair whether a falling body should, on striking the earth, remain there, and therefore have its energy transformed into heat, or rebound to an equal height. The falling body might

be absolutely free as to these alternatives and still obey the law of conservation of energy. As a matter of fact, something like this is the case. That is, all falling bodies do fulfill the conservation of energy, though they vary in all possible degrees in their behavior on striking—some rebounding to almost their original height, others hardly rebounding at all. So far as the conservation of energy is concerned then it cannot matter what happens. As a matter of fact, the same body, under the same circumstances, seems to behave in the same way. But if the same body under the same circumstances behaved *differently*, it would still be possible for it, amid all its vagaries, to obey the law of conservation; because other bodies, whose regular behavior is strikingly different from its own, still obey that law. So long as the free body imitates their behavior it satisfies the law. The conservation of energy, therefore, still leaves the body many degrees of freedom.

The question of the interaction of the psychical and physical involves the same principle. Given a psychical state *A* and a physical one *B*, what has the principle of conservation to do with their causal relation? Suppose *A* is the cause of *B*, what then? Well, says the upholder of the *a priori* argument, in that case whence comes the energy represented by *B*? The irrelevance of the question can best be brought out by an absurd answer. I will suggest that possibly it comes from the other side of the moon. Absurd, you say, why from there? What has the other side of the moon got to do with the problem? But this is an appeal to the principle of sufficient reason, which is quite another matter. So far as the conservation of energy is concerned, the hypothesis is flawless. The existence of the psychical state *A* is the necessary and sufficient condition for the transfer of a certain amount of energy from the other side of the moon to the spot of the brain in question; just as the existence at a certain point of an obstruction, is the necessary and sufficient condition for the transformation of some of the kinetic energy of a moving body into heat. Of course, we can make less extravagant hypotheses, but an extreme case of this kind serves to bring out the essential principle even better than one which is intrinsically more probable.

But an argument is never really satisfactorily answered until we have taken a sympathetic point of view. Why is it that to so many persons there seems to be a contradiction where we have not found one? What mistakes have they made in the interpretation of the principle that is responsible for their belief? If not the conservation of energy, perhaps some other principle that they have confused with it, is involved in the argument, and we have not yet got at the real inwardness of the question.

This is certainly the case, and we must follow up the matter until we have discovered the real thought involved in the theory, and decided whether its logic is straight; and, if so, whether the principle appealed to is one that we must accept with that fullness of faith that we are inclined to accord to the conservation of energy.

The idea underlying these alleged proofs of parallelism is that for *A* to cause *B*, *A* must itself be the *source of the energy* represented by *B*. When Höffding¹ says that, if a physical state causes a mental state, the energy of the former disappears, his idea obviously is that to cause is to hand on one's energy to the effect; and as the effect, the psychical fact, cannot contain the energy, the latter must disappear. That this is his thought is further shown by his immediately going on to show that it is impossible that the psychical fact should represent energy. Ebbinghaus clearly has the same idea, for after disposing, as he holds, of the freedom of the will, he goes on to discuss the only form of interaction that he regards as consistent with the law of conservation, namely, that form which regards the psychical fact as itself a form of energy. These writers have thus interpreted the modern doctrine of the conservation of energy in terms of the old ideas of causation. For them, every physical fact has a definitely assignable cause, and something passes *from* the cause *into* the effect, and that something is energy. The language is the language of science, but the thought is the thought of Aristotle.

How far is such a view necessary, or even justifiable? First, as to the general view of causality that is implied. Energy is something that belongs to things, to passive conditions. Is

¹ *Psych.*, pp. 55-56. (Eng. Translation).

causation really a relation between such things, or is it not rather between events that can hardly be conceived as representing energy? I think most scientists would say the latter. Science is coming more and more to talk of uniform sequence of events rather than of distinct causes. For science, certain events happen under certain circumstances. It is always stretching the apparent facts to say that *A* is the *cause* of *B*. For practical reasons we pick out one from the necessary conditions of the occurrence of the phenomena and call that the cause, but frequently it might as well have been another.

The inadequacy of this method of describing the order of nature may be illustrated by a simple example. A ball is thrown into the air. It rises for a while and then falls. Can we describe what takes place in the simple language of cause and effect? What, for instance, is the cause of the ball's rising? Its own motion might be suggested as the answer. But is it conducive to clearness to thus separate the motion of the ball from the ball itself? Motion in the abstract is certainly a strange *physical* cause! The energy of the moving ball is as dependent upon the mass of the ball as upon its motion. To separate them is of course perfectly possible in thought, but to ascribe to them when thus separated independent physical reality and causal efficacy is to land in doubtful territory at once. No, the only scientific way of describing the order of events is to say that a moving body does in fact always tend to keep on moving. We cannot assign a definite cause, we can only formulate the first law of motion, and thus show that a large number of cases of this kind admit of being regarded from a common point of view,—of being apperceived by each other.

We may advantageously follow the case somewhat further. What makes the ball fall? If we are to pick out another body, another material thing, as the cause, it must be the earth. But the facts are not so simple. The earth has been there right along, but the ball fell only at a particular moment, namely, when its upward motion ceased. Moreover, it would not have fallen, unless unsupported. If someone had dropped the ball, we should certainly have regarded that as the cause of the ball's falling. It dropped,

we would say, because he let go of it. But this is not all. Had the ball been lighter than air it would not have fallen, but risen, and among the causes of its rising we would have to include this fact. It rose, we would say, because lighter than air. But suppose its weight had been very near that of air. Then sometimes it would rise and sometimes fall, depending upon the height of the barometer. On the occasions when it rose, we would say it rises because the air is light to-day. Thus the cause varies under different circumstances, and it is not possible to pick out a single cause of an event which shall be in itself sufficient, let alone a cause which shall be a material thing. Science in its analysis cannot follow this popular method. It must largely give up the idea of describing everything as an effect of a particular cause, and seek to state all the conditions and circumstances under which the event in question may occur. When it does occur, it is due to the simultaneous occurrence of a variety of conditions and circumstances, no one of which may be said to be the cause.

Now if we knew the inner mechanism of all these matters, we might, perhaps, have less difficulty. We might, for example, find that the body always fell when the sum of the blows struck upon one side by certain small particles was greater than the sum of the blows upon the other side. In this case, we might point uniformly to a definite immediate cause. But, as we shall have occasion to consider more at length later on, such a cause would always be hypothetical and doubtful. We could not, probably, by its aid, point to any *first hand fact*, any object *immediately given in experience* as the cause.

If the general view of causation that seems bound up with the theory we are examining seems thus questionable, the second part of the assumption fares even worse when examined in the light of the facts. Does energy always pass from the cause into the effect? Do all the circumstances and conditions which determine the occurrence of an event contribute the energy which is transformed during the event?

The appeal must be to the facts. Here is a heavy ball at the end of a string, going round in a circle. According to the first law of motion that ball ought to travel in a straight line. Its

deviation from the straight line is due to the continual pull of the string. I do not see how anyone can avoid saying that it is the string that keeps the ball revolving in a circle, if he is going to pick out a cause at all. Yet not a bit of energy passes from the string into the ball. Indeed, a stretched string possesses very little energy. If instead of the string we used a heavy steel rod, the tension in it would represent still less energy, but it would do the work quite as well theoretically, and practically a great deal better. If the string should suddenly break, the ball would fly off, and the cause of this would certainly be the breaking of the string. But under these circumstances no energy passes from the string to the ball at all. Here there is *no transformation of energy*, but there are events, and important events, and there is causation if there is anywhere.

But it will, perhaps, be more satisfactory if we take a case where there is transformation of energy. Two balls of the same material, of equal mass, and of equal but opposite velocities, meet. A part of the energy of each is converted into heat, depending upon the value of the modulus of elasticity, and a part remains as kinetic energy as the bodies rebound. Let us consider one of these bodies. What caused the changes in it? If we are to select another body, it would have to be the body struck. Of course, this is a conspicuous case of the difficulty of selecting a *thing* as the cause, and no scientist would describe the affair in that way. The present total condition of things is the lawful result of the previous condition. The causal relation is to be found in the *event* of their striking, a purely abstract affair having no energy connected with it. But if the cause is to be found in some other thing, in some reservoir of energy, it must be the other ball. And yet to the best of our knowledge no energy passed from the one into the other. The striking against the other ball, and therefore the existence of the other ball at that particular time and place, was the sufficient condition for the transformation of a part of its energy into heat, and the change in the direction of its motion; but it received no energy from the other.

But perhaps our argument will be still more convincing, if we take a case where energy actually does pass from one body into

another, and show that even here the causal relation is not confined to the bodies taking part in the energy changes. Suppose we consider a dynamo operated by a water wheel, and supplying light for a distant town. The transformations of energy are familiar to everybody. The kinetic energy of the water is handed on to the wheel and into the armatures, and there converted into electrical energy; carried to the city, and there converted into light. The whole may be regarded as an apparatus for converting the kinetic energy of the water into light. But now consider what would happen if the field magnets were removed. The lights would go out, and the energy of the water would be dissipated into heat in the bearings of the wheels and against the idle brushes. What determines which of these two paths shall be taken by the energy, is the existence or non-existence of an electromagnetic field in the neighborhood of the rotating coil. But no energy passes from the magnetic field. The field magnets are not a source of energy in the plant. The mere passive existence of the field, the mere presence of the magnet, makes all the difference. So long as it is present, the energy passes along the useful path and lights the town; if it is absent, the energy travels along a different and useless path. The case is perfectly analogous to what those who believe in the interaction of mind and body must suppose to be the action of the psychical upon the physical. The existence of the psychical fact is the necessary condition for the passage of the energy along a certain path. If it does not exist, the energy will pass along some other path.

Now once more it must be admitted that, if we knew exactly the *modus operandi* of things, we might, perhaps, make out a case for the view that the true cause is the source of the energy. But between a view which, as scientists, we *must* accept, and one which may possibly at some future time be shown to be probable, there is a vast difference. At present, the facts do not point to any such relation. The energy seems to pass from the kinetic form into the electric in the moving wires. The electro-magnetic field seems to act like the stretched string. It is certainly not a source of energy, nor can we see that the energy really passes through it. In the present state of our knowledge, we can say

only that the presence of the electro-magnetic field is the essential condition for the kinetic energy of the rotating coil to be transformed into electric energy.

The view that the cause hands on something to the effect, that some kind of force passes from the one into the other, is not a scientific theory, but a survival of scholastic metaphysics. It is a view that would never have been suggested by strictly scientific methods of thinking. So far from supporting it are the known facts, that they distinctly indicate an opposite view. When we trace the path of *energy* (the only thing with which modern science is acquainted that seems to promise identity with the primitive notion of efficient cause), we find that path apparently determined by other conditions. The laws governing the sequence of events are not so simple as the modern Aristotelians would have us believe. The occurrence of any given phenomenon may depend upon a great many conditions and circumstances, and among these necessary antecedents there are some which do not seem to contribute to the energy of the consequent at all.

What, then, is the result of our analysis? The interaction theory is not in conflict with the doctrine of the conservation of energy, but with a particular theory of causality. This theory, when we come to examine it, turns out to be *not* the inexorable verdict of science on a question of fact, not even a point of view which the progress of science is tending to constantly press to the front, but a flagrant bit of pseudo science—a survival of obsolete ways of viewing natural phenomena, against which science has always had to struggle. This summary method of settling a great question turns out to be not a simple appeal to the “grandest generalization of modern science,” but a bit of *a priori* metaphysics masquerading in a naturalistic nomenclature.

Have we followed the question to its end? Is it not probable that deeper than we have gone there is an objection to interaction which has a healthy scientific instinct back of it? It does not seem likely that scientific men would be found to have so strong a bias in favor of the parallelistic view, if that view did not fulfill a real scientific need. The view which we have been examining may be all that we have seen, and yet be something more. We have

seen the fallacy, but where is the truth that nearly always accompanies error? There certainly is another aspect to the question, though we have completely disposed of the argument, I think, so far as the energy question is concerned.

That which really is in conflict with the interaction theory, and which has, I believe, guided all objectors, however much they may have believed themselves to be thinking about the conservation of energy, is the mechanical theory of the universe. Indeed, this view is often confused with the conservation of energy, owing, I believe, to the accident that the most conspicuous application of the former, the mechanical theory of heat, is the region in which the conservation of energy was first demonstrated, and in which it has been most completely tested.

Höfdding appeals to the mechanical theory at a critical point in his proof of automatism without, however, mentioning it as such. He is answering the argument that the effect of the psychical fact may be to change the direction of the moving particle, and therefore not affect its energy. He replies that this will not do, because nothing but a physical force can move, or change the direction of motion of, a material particle. Yet strangely enough he does not seem to realize, that if this once be admitted, there is no need of bringing the conservation of energy into the dispute. If nothing but a physical force can affect matter, why then, of course, a psychical fact cannot, by hypothesis, do so, and there is no necessity of any more argument about it. Ebbinghaus is somewhat better, in that he explicitly mentions the mechanical theory as the only bar to accepting interaction with the assumption that psychical facts are a form of energy.

Of course there cannot be any question as to the correctness of the argument. The mechanical theory assumes that there is nothing but matter and motion to be considered in the explanation of the order of nature. If it be accepted as final, and *objectively valid*, it has of course far more wide-reaching effects than the rendering impossible of interaction. I have no wish to bring these consequences into the argument, however. The religious side of the question need not concern us here.

Have we come so far only to find that in the end we must

yield? If interaction is inconsistent with scientific truth, it does not much matter whether it is the conservation of energy or the mechanical theory that compels our surrender. It is an interesting logical point, but not very important. Well, there is a difference, and it is just the difference between an observed fact and an unproved hypothesis. The mechanical theory is not, as I hope to show, a very formidable foe. It has not yet reached the stage where it is binding upon other sciences.

First of all we must make a distinction, the failure to make which is the cause, I believe, of much of the misconception that prevails in the subject we are discussing. We must distinguish, that is, between a *mechanical* and a *dynamical* view of nature. The word 'mechanical' as commonly employed has a wider and a narrower significance. In the wider sense, it is simply opposed to intelligent or teleological. In this sense, any action in which the effect is determined exclusively by the antecedents, with absolute disregard of consequences, may be said to be mechanical. In such a sense, we may speak of the mere mechanical association of ideas, as a process apparently different from the associations that go on when we are thinking intelligently. Now in this wider use of the word, there is no antagonism between the mechanical theory and interaction. The relation of antecedent to consequent may be as fixed and immutable, as utterly regardless of consequences, when these antecedents are psychical, as when they are physical. The relentless manner in which some hypnotic suggestions work themselves out suggests a very great uniformity between antecedent idea and resultant motor reaction, and in the larger sense of the word may be described as mechanical, without at the same time denying the causal efficacy of the idea as such. All scientific methods of explanation are mechanical in this sense. But by no means all—in fact, as we shall see, only a small portion—are mechanical in the more narrow sense.

In the narrower use of the word, it is equivalent to dynamical. The mechanical theory of the universe means that all laws can be reduced ultimately to the laws of motion. Not only is the relation between antecedent and consequent absolutely uniform, but it is always the relation that exists between moving bodies,

or the direct consequence of it. I shall throughout the rest of my discussion use the expression 'dynamical' instead of 'mechanical,' as less ambiguous in its significance. It is the *dynamical* view of nature *alone* that demands a giving up of the interaction theory. After this slight digression we may return to our problem. What is the difference practically, between being at variance with the law of the conservation of energy, and being opposed to the dynamical view of nature?

First, an aspect of the problem which is of interest primarily to the philosopher. The conservation of energy is simply a compendious statement of a number of directly observed facts. It is hypothetical only in so far as it extends the law beyond the limits of accurate observation. If stated approximately, it would be absolutely true—a mere statement of the results of a number of measurements of *directly observed quantities*. The law is of course not usually stated in this purely empirical form. The usual statements imply that there is a single reality, energy, whose form alone changes. This is, of course, speculative. But it is quite possible to state the law without any such assumption. We can regard energy as a *class* name, not as an individual name. The law of conservation then simply states the constancy of the sum of the quantities thus defined. When so modified, the law loses some of its attractive suggestiveness, but nothing of its importance.

The dynamical theory stands upon an entirely different footing. Except in the region of ordinary mechanics and astronomy, it deals necessarily with realities not given in experience. For metaphysics it rises or falls with the reality of atoms. For what at most can the dynamical theory show? The utmost it could prove, if it were completely worked out to-day, is that *if* we make a certain assumption as to the real nature of matter, then all its phenomena can be deduced from some given primitive state of things and the laws of motion. But it can never prove that these fundamental assumptions are really valid, or that an entirely different set of hypotheses might not serve equally well to explain the effects. Philosophy, therefore, even if the dynamical theory were an accomplished fact, and not merely an ideal, would have a

right to treat it as merely a point of view, a convenient fiction, useful for predicting events, but not in any way binding upon an investigator of other problems. The conservation of energy, on the other hand, is binding upon every shade of philosopher. It is a simple statement of the facts of pure experience as such, and cannot be ignored by any point of view, however ultimate. To philosophy then, it makes a great deal of difference whether interaction is inconsistent with the conservation of energy, or with the dynamical theory.

The scientific psychologist, however, is in a different position. He is concerned not so much with the absolute truth of the dynamical view, as with its usefulness as a method of formulating facts. In his behalf, therefore, we must inquire into the purely scientific side of the question. Have the triumphs of the dynamical theory been so great in other sciences? Is it so preëminently and exclusively the point of view adopted by all other sciences, that the psychologist is compelled to accept it at the outset, without question, in his own field?

The dynamical theory is such a beautifully simple affair, it agrees so well with our conception of the unity and simplicity of nature, it promises such elegant mathematics, such perfect clearness and certainty in our reasoning, that it is little wonder that we constantly strive to picture nature in its terms, and thus anticipate the day when this great intellectual ideal shall be an accomplished fact. But a great scientific theory must not be accepted on account of its æsthetic value merely. The more alluring it is, the more must we be on our guard. There is abroad what seems to me a most dangerous tendency to assume the whole dynamical theory, as the necessary hypothesis of science.

A most interesting, and, for us, important, case is found in the assumption, made by every one who discusses this question of the relation of the psychical to the physical, that the physical basis of mind is motion. 'Can consciousness affect the motion of matter?' is always the crucial question. That the physical condition involved is a motion is never thought worthy of discussion. Now, there is not, I believe, the least warrant for this assumption. The physical basis of mind may not be motion at all.

The pictures so often drawn for us, in which we are shown atoms moving about and taking up various positions in the molecules, and in which the nature of the different forms of energy is so confidently expressed in terms of their motion, is really little more than the scientific imagination out for a holiday. The beautiful dynamical theory is as yet but little more than a beautiful dream. In the science of physics it rises to the dignity of a working hypothesis. In other sciences it is at most a hope. Personally I share that hope, but I do not see that that entitles me to call another man unscientific, because he may prefer to attempt the formulation of phenomena in other terms. For, if we rigorously exclude from our thinking all *a priori* elements, and all the prestige which the dynamical theory obtains from its simplicity, and readiness of dogmatic interpretation; if we exclude, too, hardest task of all perhaps, the attraction that lies in its vigorous negation of the pseudo miraculous—if we confine ourselves to the calm consideration of the *actual evidence* in favor of the dynamical theory, we shall find that, thus boiled down, it amounts to extremely little. It has been actually worked out in only a portion of the single science of physics. For we must remember that the evidence for the view must be drawn from phenomena that are not apparently and obviously dynamical in their nature. Astronomy, for example, deals with phenomena obviously dynamical, and, while the proof it affords of the spatial universality of the laws of motion and of gravitation is most important, it can hardly, it seems to me, be urged as evidence that phenomena of a *totally different character* are also dependent upon motion. The same holds, of course, for what is commonly known as mechanics—the laws of falling bodies, the theories of the structure of buildings and bridges, etc. In the domain of sound, we first begin to find applications of dynamics of the kind necessary for founding the dynamical theory of nature. But even here the evidence is hardly ideal. Sound is a case, largely, of molar motion. The sounding body may often be actually seen to be in motion, and it is this actually observed motion which gives rise to the sound. In the discussion of the conduction of sound also, we are largely concerned with those properties of bodies which

are directly observed, such as their mass, elasticity, etc.—properties which, according to the dynamical theory, are due to molecular motions and attractions, but which *in the theory of sound we mostly assume as given*. In brief, the theory of sound furnishes no evidence of that view which is at the very foundation of the extension of dynamics to all the phenomena of nature—the view, namely, that the properties of matter not obviously dynamical are really due to the dynamical relations of hypothetical atoms. Evidence in favor of this essential point in the dynamical theory is very slight. Boiled down, it amounts to little more than the kinetic theory of gases. Now, I have no desire to make light of this great achievement. I fully admit the tremendous prestige which this great conquest quite rightly gives to the hypotheses and general points of view by which it was gained. Nevertheless, we must not forget that after all the kinetic theory of gases is only a *single* chapter in the *single* science of physics.

There is some tendency, I think, to regard every application of mathematics to physics as evidence for the dynamical theory. This is not justifiable. Much of mathematical physics is really not dynamical at all; much of the mathematical theory of heat was developed before the dynamical theory of heat. Most of the mathematical treatment of light has nothing to do with the view that it is really a vibration; it deals only with its periodic character, and is consistent with any theory of the real nature of light which recognizes this periodic character. Still more of mathematical physics is of the character spoken of above in connection with sound. It deals with the known properties of bodies, and not with their supposed atomic causes. Science is, in fact, much more ‘positive’ than the dynamical theory. Mathematical physics does not deal with molecules nearly as much as popular physics; and when it does try to, it has by no means been uniformly successful. Now much of this non-success of molecular physics is perhaps due to the inability of mathematics to cope with the problems presented by it. On that account we are justified in retaining our *faith* in dynamical ideas if we choose to, and, in some respects, at least, I, for one, do so choose. But surely the mere fact that we can *explain away* the lack of striking progress

in molecular dynamics, is no adequate ground for forcing the theory on all comers and upon all sciences! The most dangerous theory for a science to have anything to do with is one that can be neither proved nor disproved, and the dynamical theory, propped up by our ignorance of mathematics, comes dangerously near that condition.

If the usefulness of the dynamical theory is thus doubtful in some portions of physics even, its usefulness in other sciences is subject to still graver doubts. The science of chemistry, for example, which for beauty and accuracy of reasoning, rapidity and healthiness of growth, and even in industrial importance, almost, is certainly not second to physics—this whole great science has never derived anything from the dynamical theory. The internal dynamics of the molecule is a *terra incognita* to analytic mechanics. We do not know whether Newton's laws of motion apply to the motions of atoms—we do not know even that atoms move at all. A totally different line of hypotheses may be necessary to explain the laws of chemical combination. When so eminent a chemist as Dr. Ostwald, a man who is an accomplished physicist as well, gives it as his deliberate opinion that the dynamical theory has outlived its usefulness, we may well be surprised to see the young science of psychology propose to assume the entire theory as its starting point! It is interesting to notice in this connection, that whereas the writers whose views we have been considering seem to find in the conservation of energy a sort of proof of the dynamical theory, Dr. Ostwald looks to the idea of energy to replace the dynamical theory.

We are now in a position to return to our original problem, as to whether it makes much difference to scientific psychology whether interaction is inconsistent with the conservation of energy, or with the dynamical theory. The conservation of energy is a principle that, in so far as it applies at all, applies to all classes of phenomena. Chemistry and physiology alike with physics own its sway. To deny its application to brain action by taking advantage of the fact that the experimental error is so much larger in physiology than elsewhere, would be a very questionable procedure for a psychology that aims to be in line with

natural science. But with the dynamical theory it is otherwise. Brain action, considered from a purely scientific standpoint, without regard to its peculiar psychical concomitants, belongs to a group of phenomena to which the dynamical theory has never been successfully applied. All the analogies of physiology point to the action of the nervous system being primarily chemical, and chemistry, as already pointed out, has not developed along the lines of the dynamical theory. The behavior of the molecule regarded as a unit has yielded somewhat to dynamical investigations. The behavior of the *atoms within* the molecule has never been expressed dynamically. The laws of chemical combination and dissociation have not been formulated in this way. Here, again, the striking difference between the conservation of energy and the dynamical theory comes out. The consideration of the energy relations has been most useful in chemistry—the dynamical theory, barren of definite results.

Once more let me insist that I am not attempting to prove that the dynamical theory has outlived its usefulness. I am simply protesting against the dogmatism that would represent the theory as already proved—that would make a point of view that happens to be very common in physics, the necessary presupposition of all science. There is a tendency to-day to regard the working out of the dynamical theory as the sole problem of science. There are persons who will readily admit all the claims here made as to the small amount of actual proof forthcoming; and fully admit that in any case it is only a convenient fiction. But they will claim that by science we mean the effort to interpret phenomena in terms of this particular fiction, and that, therefore, by definition all science must be dynamical. I cannot but think that this view is largely the result of a failure to distinguish between a mechanical and a dynamical theory. That a mechanical point of view, in the large sense, is necessary for science, I am willing, for the sake of the argument, to admit. Granted that except where the consequents are determined solely by the antecedents, and according to absolutely uniform laws, there is no field for science; it does not, therefore, follow that the nature of that uniformity must be expressible in dynamical terms. Such a view leads to

conclusions that are absolutely untenable. According to this view, we have to-day no science with the exception of a small portion of physics.

The other sciences are called such only by courtesy. They are sciences in the same sense in which every western village is a city—may some day become one. Similarly, all the so-called great scientists, with the exception of a few, have not really contributed to the advance of science as yet. All we can say is, that, if the advance of knowledge continues long enough, their work will some day bear fruit; will be seen to have been a necessary stage in the development of a science. The Darwinian theory, for example, is only a useful semi-scientific formulation of the order of events, that is paving the way for a future science of biology. If through any accident the advance of science should be checked, this theory, though interesting and useful, would have no scientific value.

Of course it is not possible to accept any such definition of the word science. It would be absolutely contrary to universal usage. But the question of names aside, the fact remains that the dynamical theory is not necessary for the advancement of those branches of knowledge with which psychology is most closely connected, and whose success in introducing order and system into the chaos of phenomena she emulates. It is still an open question whether all phenomena can be expressed in terms of the dynamical theory or not. Such being the case, the value of knowledge that systemizes facts in other ways cannot be questioned, nor a merely temporary value assigned to it. So far as we can see at present, we are as likely to hinder, as to advance, the organization of experience, by accepting the dynamical hypothesis in full. Thinkers who regard the dynamical theory as an end in itself will of course risk this chance of wasting their efforts, and abide by the results. But those of us who regard the dynamical theory only as a means to an end, will want better proof that it is the surest and best means for attaining that end, before we definitely and finally close the door to other hypotheses.

Psychology to-day accepts the parallelist hypothesis, not be-

cause it is in harmony with the dynamical theory, but because it accords strikingly with the facts of cerebral physiology. It is a working hypothesis to be used as long as it proves useful, and discarded as soon as it comes in conflict with facts. It is not the starting point, but the first and most important big *result* of physiological psychology, a growth from within, not an importation from physics. The *a priori* proofs turn out, upon examination, to be without adequate foundation, an outgrowth of a confusion between two scientific principles, liberally mixed with metaphysics of a very questionable kind. For the psychologist of the present, it makes very little practical difference, perhaps, whether parallelism be an imported or domestic commodity. It is the accepted hypothesis of his science, and will probably remain so for many years to come, in any case. But to the philosopher, it makes a great deal of difference; and to the psychologist of the future, it makes a great deal of difference. It is always hard enough to overthrow a theory that has been accepted for a long period, even when it boasts no higher pedigree than is furnished by its accord with a large number of facts within the particular branch of science in question. How much harder then will it be to go back to the interaction theory, should the facts demand it, if the next generation are allowed to grow into the conviction that science is impossible save upon the parallelistic hypothesis! It is never safe to let an error masquerade as truth whether we see any immediate bad consequences or not. To many, interaction may seem a possibility so improbable as hardly to be worth the saving, and the preceding discussion a mere question of logic. I cannot accept such a view. When I reflect that perhaps none of the guesses at the riddle of existence that have thus far been made may be anywhere near the truth, that the universe is perhaps a vastly bigger thing than our present knowledge dreams of, it seems to me to be of the greatest importance that every possible theory of life, every view to-day tenable, should be kept open. Interaction seems to me to be one of these *tenable* views, and, as such, worth defending, and keeping in the background of one's mind as a possible aid when more popular theories fail.

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